ART. IV. — 1. *Annales de l' Observatoire Impérial de Paris.* Publiées par U. J. LE VERRIER, Directeur de l'Observatoire. Paris. 1854 – 59. 5 vols.

2. *Auseinandersetzung einer zweckmässigen Methode zur Berechnung der absoluten Störungen der kleinen Planeten.* Von P. A. HANSEN. Leipsic. 1856 – 1859.

3. *The Astronomical Journal.* Edited by B. A. GOULD, Ph. D. Vols. I. to VI. Cambridge. 1849 – 61.

4. *Tables of the Moon.* Arranged in a Form designed by PROFESSOR BENJAMIN PEIRCE, under the Superintendence of CHARLES HENRY DAVIS, Lieut. United States Navy. Washington. 1853.

5. *Tables de la Lune construites d'après a la Théorie Newtonienne de la Gravitation Universelle.* Par P. A. HANSEN. London. 1857.

We bring these works together as illustrative of modern theoretical astronomy. Were Hipparchus or Ptolemy to rise from the dead, and turn over their pages, he would fail to perceive any indications, other than the occasional name of a planet or a star, that the works treated of the science in the pursuit of which he spent his life. Could he observe the men who wrote them engaged in their scientific avocations, astronomy would be the last branch of knowledge to the cultivation of which he would assign them. So complete a revolution has been wrought in this science by the reduction of all the important celestial phenomena to exact mathematical laws, that a man may rise to the highest eminence as an astronomer without being able to tell one star from another, and without thinking of the heavens otherwise than as they are contained in his mathematical formulæ. In order that we may clearly understand the relation in which purely theoretical astronomy stands to other branches of the general science, let us take a brief view of the different classes of operations in the construction of a perfect inductive science. These may be enumerated as follows.

1. The collection of facts derived from experience. These will form an exact history of the phenomena relating to the science, and have indeed been included by Bacon in the general

term Natural History. The natural history of astronomy consists of observations of the positions and aspects of the heavenly bodies.

2. The inference, by collation of and induction from the facts of natural history, of general laws, and the construction of hypotheses respecting the causes of phenomena. Lord Bacon has given a body of rules for determining systems of empirical relations among phenomena. But these alone are not sufficient for all the purposes of science. It may be advantageous to know, in addition, the mechanism by which such relations are established, or, in other words, the proximate causes of those relations. An exact analysis of the distinction between phenomenal laws and their causes is not within the scope of the present paper, and we shall only remark, that the theory of gravitation properly comes under the former class, since it does not assign any cause for celestial phenomena, but only reduces them to an empirical law.

3. The next process is one of *deduction*. Starting from the general principles which have been inferred or guessed at, in the operation of induction, we determine by rigorous reasoning the consequences which flow from them, and thus attempt the prediction of as great a variety as possible of unknown phenomena. The deduction must be made, if possible, by mathematical reasoning, in order that the results may be given in *quantity* as well as in kind. Modern science is not satisfied with general statements respecting the kind of result; it also demands, How much? And before an hypothesis can be received, its results must agree in quantity as well as quality with those derived from experience.

4. Finally, the consequences which the mathematician or other deductive reasoner has found to flow from the hypothetical principles are compared with observations, and their agreement or disagreement noted. If the agreement is perfect within the unavoidable errors of observation, the hypothesis may be regarded as an established theory; if not, it must be modified or rejected. In the former case, the theory will form the basis of the whole or part of an exact science, and we may then be able to predict phenomena with as much certainty and accuracy as we can observe them.

It is principally to the deductive branch of astronomy that we propose to call attention in the present article. It is developed by Le Verrier in the *Annales* and by Hansen in the *Auseinandersetzung*, in the most nearly perfect form to which human ingenuity has yet succeeded in bringing it. Consisting as it does almost entirely in carrying out the results of the law of gravitation, it might almost be considered a branch of pure mathematics. The problem may be expressed as follows. A certain number of bodies, one of which is very much larger than all the rest, start from given positions, with given velocities, and in given directions, and are then left to their mutual attraction, each being supposed to attract all the others according to the law of gravitation: — to find general rules for calculating their motions during all time. The solution of this problem may be regarded as one of the greatest triumphs of the human intellect. Nothing can be conceived of more hopeless than an unaided attempt to solve it on the part of any one man, even of the highest genius. He might pass from his cradle to his grave in constant efforts toward this end, without seeing himself any nearer a solution than when he started. Patience and perseverance may, indeed, remove a mountain of sand; but the task we have supposed would be like the removal of a mountain of solid rock, which, when raised at all, must be raised by a single effort. It was not till nineteen centuries after Apollonius discovered the properties of the conic sections, and four centuries after the revival of letters in Europe, that the mind of man succeeded in inventing an instrument by the aid of which it could grapple with the problem. Simultaneously with the invention of the calculus came the discovery of gravitation; and even then, with the problem and the instrument for its solution clearly before them, three generations of mathematicians were required to complete the outline of the solution. The method was finally developed, with as much rigor as the problem admitted, by Lagrange and Laplace, in the latter part of the last century. It is explained with much detail, and with great rigor and accuracy, in the first and second volumes of the *Annales de l'Observatoire*, in such a way as not to require in the reader a more extended knowledge of mathematics than may be obtained from college text-books published in this country.

The solution thus presented is general; that is, all the perturbations in the motions of planets are expressed algebraically, and all the quantities that differ in different planets are expressed, not by separate numbers for each planet, but by algebraic letters which include them all. By substituting for these letters the numbers which relate to any particular planet, we ascertain the perturbations of that planet. Unfortunately, however, the possibility of this solution depends on two assumptions, — first, that the planetary orbits are nearly circular, and, secondly, that they are nearly in the same plane. Both these assumptions hold good as to the larger planets and their satellites. But the present century has witnessed the discovery of that immense group of "asteroids," most of which are extremely eccentric, and wander beyond the limits of the zodiac. Here the general solution is no longer practicable by any method yet discovered, and we are obliged to confine ourselves to an arithmetical solution for each asteroid separately. To develop the most improved and exact way of doing this is the object of Hansen in the *Auseinandersetzung*.

It is a little remarkable, that, if we take a geometrical view of the mathematical formulæ which give the position of a planet, we shall find them to represent systems of epicycles analogous to those of Hipparchus and the Greek philosophers. It will be remembered that these early speculators represented the motions of the planets by the conception of a movable circle rolling on the circumference of one that was fixed. A point on the circumference of the moving circle represented, approximately, the motion of a planet. The modern mathematician

> "Girds the sphere
> With centric and eccentric scribbled o'er
> Cycle and epicycle, orb in orb,"

to a degree of which the ancients never dreamed. He constructs a system of moving circles and ellipses, of which the number is theoretically infinite. The centre of a large circle coincides with that of the sun, while its circumference is at a distance equal to the mean distance of the planet. Around this circumference moves the centre of a small ellipse; around the perimeter of the latter revolves the centre of a second ellipse, still smaller; and so on to any extent. From fifty to two hun-

dred ellipses are practically sufficient, as all beyond are so small as to be inappreciable. The planet is supposed to move around the perimeter of the last ellipse.

It is impossible, by any known mathematical methods, to reduce the solution to a more simple form. So complex a motion might at first sight seem incompatible with the simplicity which characterizes the laws of nature in general, and that of gravitation in particular. But it must be remembered that this simplicity pertains to the *causes*, not the effects. A few of the most simple causes may, by their numerous successive combinations with the various conditions under which they operate, produce the most complex effects. A child can comprehend Boscovisch's atomic theory of matter; no mortal mathematician can trace its consequences, else it is quite possible that chemistry would cease to be an experimental science.

One effect of the perfection of theoretical astronomy has been to render the ancient observations of the positions of the planets nearly valueless. In order that the testimony of an observer may be of any value, he must be able to tell us what he saw with more exactness than we can determine it ourselves. But with respect to most of the ancient observations respecting time and position, — the two great astronomical elements, — a mathematician of the nineteenth century can tell what the observers saw better than they could tell themselves. Of a phenomenon of which they can give only the hour, he can determine the minute. Owing to uncertainties respecting the changes in the orbit of the moon, solar eclipses still furnish an exception to this rule; but there is little doubt that, after another century, we shall be able to say of the astronomer,

> "Past, present, future, to his sight
> At once their wondrous scenes display,"

as well with respect to eclipses as to other celestial phenomena.

One of the most interesting questions which has arisen from the investigations of modern science in the general laws of nature, is that of the stability of our universe as at present constituted. Is this system fitted to run on forever, in accordance with its present laws, or will these laws,

in the end, lead to its subversion? The conclusion was reached by Laplace, and has been confirmed by subsequent investigators, that, so far as the force of gravitation alone is concerned, the system is stable. Every change which the attraction of one planet produces in the orbit of another will finally induce its own compensation, and bring the system back to its original state. But the discoveries of the present century respecting the correlation of the different forces of nature, the conservation of force in general, the nature of the solar light and heat, and the motions of the comets, seem to indicate that gravitation is by no means the only force by which the motions of the heavenly bodies are influenced, and that causes which slowly, but surely, undermine the system are in operation; — that the latter is not, therefore, a self-winding clock, which may run forever, but that it must ultimately lose all motion, unless some power, capable of controlling the laws of material nature, shall interfere to preserve it. We shall give some examples of these destructive forces.

In the first place, the sun is radiating heat into space in quantities incomparably greater than it receives. If it were not so, we should receive, on the average, as much heat from every other quarter of the heavens as from the sun, and no vicissitudes of temperature could ever occur on the earth. From what we know of the nature of heat, it is impossible that the supply contained in the sun should be absolutely infinite. The sun must, therefore, as centuries advance, grow cooler and cooler, until its heat is entirely lost. This will be followed by the cooling of the earth, and thus all life on our planet must cease, or the conditions of its existence must be completely changed. It may be asked, Is it certain that the heat of the sun is not returned to it in some other form? It is, of course, impossible to give any absolute and direct proof that the sun does not receive heat, or its equivalent, from some unknown source; but it is certain that we can trace the operation of no natural law which would tend to return heat to the sun, and that the existence of any such operation seems improbable. It has been suggested, that the sun may be supplied with fuel by comets or other bodies falling into it. But there are two objections to this hypoth-

esis, either of which would be fatal. In the first place, a supply of some supporter of combustion, such as oxygen, is as necessary as is a supply of combustible matter. In the next place, it is impossible that combustible material should fall into the sun in sufficient quantities to keep up its supply of heat; for a mass of bituminous coal of the size of our globe, would, by its combustion, supply the sun for about thirty days, and the largest comet would not furnish it with fuel for an hour. The mechanical force with which a body as heavy as our globe would fall into the sun from an infinite distance would, if converted into heat, supply a quantity of this agent sufficient to last the sun only about sixty years. If, then, the heat of the sun is kept up at all, it must be by means of some invisible influence, inscrutable to mortals; a supposition which we may well reject, in view of the fact that we have no more reason to suppose that the Deity intended the sun to be eternal, than that he intended the earthly life of man to be so.

Another element of destruction probably exists in the form of a very rare resisting medium. It is true that the existence of such a medium is not yet demonstrated with certainty, yet we have some evidence in its favor. There is no reason, *a priori*, why we should suppose the planetary spaces to be perfectly void; on the contrary, the general analogies of nature would lead us to suppose that they still contain something material. Now there are two classes of phenomena which point to the existence of an ether, filling all space, and possessing the property of inertia. These are as follows.

1. The phenomena of light and heat. These seem to be due to a vibratory or oscillatory motion among the molecules of an ethereal medium. By the heat-vibrations force may be communicated from one body to another distant body having no material connection with it; it is therefore concluded that the ether is possessed of the property of inertia. In our ignorance of the exact nature of its motion, and the amplitude of its vibrations or oscillations, we have not sufficient data for determining the density of the hypothetical medium. But this density, however small, must be appreciable, and therefore retard the motions of all bodies moving through it.

2. The observations of Encke's comet made during the last thirty or forty years show that its motion is continually undergoing acceleration * from some cause, and that, if this continues, it will in a few centuries fall into the sun. This comet, being a small nebulous mass of excessive tenuity, is precisely the object which would be most affected by a resisting medium, and Encke attributes its acceleration to this cause. His view is controverted by other astronomers, some of whom attribute the anomalies of the comet to the repellent action of the sun in driving off the comet's tail, — a subject to which we shall presently revert. In view of these controverted points, it will be hardly fair to consider it certain that the motions of the planets will ever be affected by the ether, especially as it is possible that, even if the ether exists, it may not affect their motions.

Yet another cause, slowly producing an entire change in our earth, is found in the mutual action of the moon and the tide-wave. As the latter glides over the oceans, and rushes into the numerous indentations of the coast, the motions which it produces in the waters necessarily involve an expenditure of power, or *vis viva*, in overcoming the effects of friction. The *vis viva* thus expended must be drawn from the set of machinery which produces the motions, that is, from the motion of revolution of the moon and the motion of rotation of the earth. It cannot be returned to this machinery, because all that is not spent in triturating the sand or other material which forms the bed of the ocean is turned into heat and radiated off into space. Its loss will manifest itself in exactly the same way in which a resisting medium would take effect; that is, the motion of revolution of the moon will be accelerated, and the rotation of the earth retarded, till the day and the lunar month become equal.

The action of such a cause is traceable in the rotation of the moon on its axis. It is well known that our satellite has always, since the earliest records of its appearance, presented

* It may appear paradoxical that a resisting medium should cause the motion of a body moving through it to be accelerated. It produces this effect indirectly. If the medium retards the body by the smallest amount, the latter will fall slightly toward the centre of attraction, and the increase of velocity caused by this fall will more than compensate for the retardation which produced it.

the same face to the earth. It is in the highest degree improbable that its rotary motion was in the beginning exactly adjusted so as to produce this effect. But if the moon were liquid, or covered by a liquid, the immense tides produced by the earth would in time produce the effect which we now see. The hypothesis that the equality in the times of revolution and of rotation of the moon is to be attributed to this cause, derives additional strength from the fact that the satellites of Jupiter seem to follow the same law. This equality, once established, will continue forever. In consequence of the acceleration of the moon's motion and the consequent diminution of the lunar month, she will in a few thousand years be half a month ahead of the place in which she would be if her month were to remain constant, and will therefore be on the opposite side of the earth. If her time of rotation remained constant during that period, the side which is now hidden would then be presented toward the earth, so that our posterity of two hundred centuries hence would have an opportunity of unveiling its mysteries. But analysis has answered the question for them, and demonstrated to us that the hemisphere of our satellite which is now turned from the earth will so remain, hidden from mortal eyes, to eternity. Poets then may let the imagination run unlicensed over its scenes of ineffable beauty, and people it with giants or genii of any order of magnitude or intelligence, walking through Elysian Fields of boundless extent; or they may make it into Tartarean depths, compared with which the fiery gulf in which Satan and Beelzebub rolled with their "horrid crew" will sink into insignificance, without the slightest apprehension that the most remote posterity will ever see anything inconsistent with their descriptions.

Viewing the system of the world, then, with the aid of all the light that can be thrown on it by science and by philosophy, selecting the most probable causes for those phenomena of nature which we cannot deduce from the known general laws of the universe, and tracing these and all known causes to their most remote and latent effects, — making at every step all due allowance for our ignorance, and giving proper weight to every sound philosophical principle which bears on either side, — there seems to be a decided preponderance of

evidence in favor of the doctrine that this system is not entirely self-sustaining and self-compensating, but is subject to actions which must lead to its ultimate subversion. We may compare the counter effects of the sustaining and destroying processes to the chemical and physiological actions which are at play during the life of an animal. On a first examination of the latter, with respect to the chemical actions involved in it, we should find it composed of materials subject to speedy decomposition, — wasting from every pore, liable to a thousand accidents which would insure its speedy dissolution, and to all outward appearance destined, under the most favorable circumstances, to live but a few hours. A more extended examination into the laws and conditions of its existence, into the operations of those organs whose office it is to repair the waste by the admission, digestion, and assimilation of food, and into the means of counteracting the effects of all destructive chemical and mechanical action to which the animal would ordinarily be subjected, would seem to show that these objects were perfectly fulfilled, so that every day might find the animal in exactly the same bodily state in which it was left on the day previous. As far as physiology could determine, the animal might continue to exist forever. Yet, if we were able to perceive the intimate mechanism by which the operations of repair were effected, and not its correlations with the causes of decay, and could trace the manner in which each new particle replaced the previous one, we should find certain imperfections in the operations. We should find that every successive cell, by a necessary law of the organism, was in some way different from the previous one; and although the progressive changes thus produced in the organism might at first sight seem insignificant, a careful examination would show that they would necessarily, at some future time, lead to the dissolution of the animal.

So with the cosmos, the first thought of a system of bodies moving among one another in every direction, and entirely given up to their mutual attractions, would lead us to apprehend that by virtue of this attraction they must speedily fall into one another, and form themselves into a single mass. But a careful examination, by mathematical analysis, of the combined effects of the destructive force of gravitation and

of the motions with which the bodies were originally endowed, shows that these are so combined that under their influence alone the system might continue to eternity. But, as we have already shown, when we extend our investigations beyond the law of gravitation which regulates the great and salient phenomena of the universe, to the less obtrusive mechanisms discovered by a more minute examination of the lesser phenomena, we find little leaks of power, which, unless counteracted in some way not yet discovered, must ultimately lead to subversion.

Another cosmical question which theoretical considerations have greatly aided us in limiting is that of the infinite extent of our system of stars. To the reflective astronomer, as he sounds depth after depth of starry systems, to all appearance bottomless, no subject of speculation would appear more attractive. As in all other questions which we are not able to solve by direct experiment, we must begin by asking what consequences would follow from the affirmative and the negative of the question respectively. Starting from the hypothesis that infinite space was scattered with stars, mathematicians had no difficulty in proving that, unless the light were absorbed in its passage through space, the whole celestial vault would be one blaze of light, brilliant as the noonday sun, on which the moon and planets would appear as dark patches. It was therefore concluded by Chéseaux and Olbers, that the celestial spaces probably contained some ether which possessed the power of absorbing light. This theory was subjected to a test by Struve, in his work entitled *Etudes d'Astronomie Stellaire*, in the following manner. If the stars are equably distributed through space, and are of equal absolute brilliancy, the number of stars of each magnitude would be at least four times as great as that of the next larger magnitude, supposing that no light were lost. An extinction of light would lessen the proportionate number of small stars. Now, this is precisely what is found to be the case; whence it is concluded, either that the stars are more numerous in the neighborhood of our system, or that light is absorbed. Considering the former horn of the dilemma very improbable, Struve adopts the latter, though it cannot yet be considered as an established theory.

One object of Struve's investigation was to show that the idea of an infinite universe was not incompatible with the appearance of the heavens. But this is not the only difficulty to which the hypothesis of such an infinite universe as we have supposed would lead. Unless heat as well as light is absorbed, we should experience a temperature compared with which that of a reverberatory furnace would be as the frozen pole. The principal difficulty, however, would be that resulting from the attraction of the infinite mass of stars. The attractions of the different parts of such a mass could not counterbalance themselves everywhere, and some systems would be exposed to an infinite attraction. True, it is difficult exactly to define what stars would come into this category. At first sight it might appear that, since each star is equally surrounded by an infinite series of other stars, each ought to be equally attracted in all directions. This conclusion would be correct, if the combined attractions of the more distant stars gradually diminished, so as to vanish at infinity. But, although the attractions of the separate bodies do diminish as we increase the distance, yet the entire number which will be contained in a spherical surface, at any given distance, will increase in the same proportion that the attraction will diminish, so that the combined attraction will not vary at all. Now, if we examine the reasoning on which the conclusion cited above is based, we shall find that it tacitly assumes that for every attracting mass of stars on one side of any star, taken at pleasure, there is an equal attracting mass on the other side to counterbalance it. We thus profess to compare two infinite magnitudes, and pronounce them absolutely equal. But two magnitudes can be pronounced absolutely equal only when certain relations exist between their boundaries. Now, by hypothesis, our magnitudes are infinite, therefore without bounds, and therefore without means of comparison, so that the whole reasoning is illusory. Moreover, it is mathematically demonstrable that, if the stars in any one position were in an equilibrium as to the opposing forces, this could be the case in no other position.

It must be understood that we have thus far spoken of the infinite system of stars as scattered indiscriminately, but with

a certain approach to uniformity, through space. But the hypothesis of an infinite increase does not necessarily involve this arrangement, or any of its attendant difficulties. We need only suppose, with Lambert and others, that the mode of formation which we see carried out in those portions of the universe visible to our eyes is continued to infinity, that out of a proper number of systems of a lower order systems of a higher order are formed, and that the separate systems are always placed at vast distances compared with the dimensions of the system. The lowest systems in this series are composed of a planet, with one or more satellites, the dimensions of which, astronomically speaking, are inconsiderable. The separate planets are formed into the solar system, being placed at distances of hundreds, or even thousands, of millions of miles. The fixed stars, which are supposed to be the centres of solar systems like our own, are placed at distances so great that the entire dimensions of our solar system are but a point in the comparison. There may be great numbers of other starry systems or milky-ways like ours formed into a system, a collection of these systems into another, and so on, without end. Of course we are now in the domain of pure speculation, as all systems of a higher order than those composed of individual stars must remain forever invisible to mortal eyes, and while man dwells on our planet he has no more means of becoming acquainted with their existence than he has of seeing the inhabitants of Neptune. The subject may therefore be dismissed with the remark, that the arrangement is not, so far as can be seen, carried out with perfect regularity. Other starry systems seem to merge insensibly into clusters forming part of our Milky-Way.

A few words with regard to late efforts to make "our refractory satellite" travel in the paths prescribed for her by algebraic formulæ. In speaking of the perfection to which Lagrange and Laplace brought the theory of the system of the world, an exception should be made of the theory of the moon's motion. Although Laplace found a number of observed inequalities to result from the law of gravitation, and materially lessened the discrepancies between theory and observation, there were yet some quantities the computation

of which he found so complicated that he concluded it to be entirely impracticable, and had recourse to observations to determine their value. Damoiseau seems to have been the first who attempted to construct tables of the moon founded entirely on the theory of gravitation. These were published in 1824; but, as they never displaced those of Burckhardt, published in 1812, they cannot be regarded as successful. The next laborer in this field was the celebrated Hansen of Gotha, of whom we have already spoken. In a work, published in 1838, entitled *Fundamenta nova Investigationis de Orbitæ veræ quam Luna perlustrat*, he gave a general method of computing the lunar inequalities from the theory of gravitation. From that time till 1857 he seems to have been principally occupied in the performance of the numerical computations which he had indicated in this work, with a view to the construction of a set of lunar tables which should excel all previous ones in fulness and accuracy. So long and intricate was this process, that, notwithstanding the patience which could devote nineteen years to such a task, Hansen would have found it impracticable but for the assistance of the British government. Through the influence of the Astronomer Royal, the Lords Commissioners of the Admiralty furnished Hansen, from time to time, with considerable sums for the employment of assistance, and when the work was finally completed, they undertook the entire expense of its publication, and liberally distributed copies throughout the world.

About the time that Hansen commenced the construction of his tables, Professor Airy undertook a somewhat different investigation, with the same end in view. This was nothing less than the reduction of all the lunar observations made at Greenwich from 1750 till 1830, and their comparison with the most approved theory, in order to discover what differences still existed between the theoretical and observed positions. The theory adopted was that of Plana, Hansen having not yet made the numerical application of his method. One of the principal results arrived at was, that an inequality which no astronomer had yet found to ensue from gravitation, and on the very existence of which doubts had been cast, did undoubtedly exist. Airy wrote to Hansen, requesting him to

make this inequality the subject of special investigation. This astronomer finally found that two inequalities, each having a period of about two hundred and forty years, would result from the attraction of Venus, and on calculating their amount, and correcting the theory of the moon to accord with them, the discrepancies which had for half a century bade defiance to all the efforts of the mathematician were found to disappear. The satellite was right, after all, as she had been found to be in former contests, and she now performs her monthly course in exact accordance with the theory of gravitation.

On the establishment of the American Nautical Almanac in 1849, one of the most important objects was the preparation of the Lunar Ephemeris. The reduction of the Greenwich observations, to which allusion has been made, was completed by Airy, and the results had just been given to the world. The discoveries of Hansen respecting the newly discovered inequalities were now at the command of the astronomer; but a painful doubt still existed respecting the time when his Tables of the Moon would be completed. All the large European Ephemerides still used the tables of Burckhardt, forty years old, and, though the best systematic set yet published, giving places of the moon so erroneous that their continued use has been lately pronounced a disgrace to science. Under these circumstances, it was determined to make a new set, based on Plana's theory, which had been used by Airy in computing the theoretical places of the moon for the purpose of comparison with observation, and adding such terms as were shown to be necessary by the theoretical investigations of Hansen and the comparisons of Airy with observation. The object which would thus be attained would be the prediction of places of the moon very nearly exact, rather than of those places as given by a single consistent theory. This diminishes their value in a purely scientific aspect, because, unless a set of tables is constructed on the last-mentioned principle, their comparison with observation can neither lead to any very definite result respecting the corrections to be made in the fundamental elements of the theory, nor throw any light on the question whether any unknown sources of disturbance affect the motion of the moon.

It does not appear that the American tables were expected, at the time of their construction, to continue sufficiently accurate for permanent use; but they have thus far passed well through some severe tests. They have furnished data for the prediction of solar eclipses, with unexampled precision. The average differences between the positions of the moon observed at Washington and Greenwich, and those given by the tables, during 1856, 1857, 1858, and 1860, are as follows: —

1856 . . .	$3''.5$	1858 . . .	$3''.6$
1857 . .	2.8	1860 . .	3.7

The absolute errors of the predicted places must, however, be less than this, because it is impossible to observe positions of the heavenly bodies with entire accuracy. The ephemeris computed from Hansen's tables for 1852 has been found to agree with observation about as well as that from the American tables in 1857, and therefore better than the American tables in other years. No complete comparisons of those tables with observations made in years subsequent to 1852 have yet been published; we have, therefore, no data for comparing the practical values of the two tables.

To give an idea of the absolute accuracy of these modern tables, and the fidelity with which they furnish positions of the moon, we may remark that the smallest round object visible to the unassisted eye subtends an angle of about a minute, and that two such objects will to an ordinary eye seem like a single object if their distance apart is less than three minutes. To the naked eye, the two stars ϵ *Lyræ** present the appearance of a single star somewhat elongated. Their distance apart is somewhat more than three and a half minutes. Yet this seemingly inappreciable space must be divided into sixty portions in order that each portion may be equal to the average difference between the real position of the moon and that predicted from theory. Notwithstanding the apparent slowness of the diurnal motion, the time of rising of the theoretical moon will very seldom differ half a second from that of the real one.

An example of the amount by which a planet must wander from its assigned orbit to produce a commotion in the astro-

nomical world, is furnished by those anomalies in the motion of Uranus which led to the discovery of Neptune. After being for thirty years a source of perplexity to astronomers, they were measured with such accuracy as to indicate to within a degree the direction of the planet producing them. Yet, if two stars visible to the naked eye had moved through the heavens during those thirty years, one keeping in the position of the actual Uranus, the other in that of the theoretical Uranus, the naked eye could at no time have perceived any indication that the two did not form a single star.

We have intimated that every anomaly of the moon seems at length to be satisfactorily accounted for by the theory of gravitation. But the last few years have given birth to a most singular mathematical controversy, which may reasonably justify a suspension of judgment on the point to which we have referred. Common consent would no doubt place mathematicians at the extremity of the scale of agreement directly opposite that assigned by the proverb to doctors. But common consent must revise its notion of the remorseless rigor of every step in every course of mathematical reasoning; for we now have the singular spectacle of half a dozen of the greatest of living mathematicians disputing for years on a point of pure mathematics. With the possible exception of an attack by Mr. Ivory, some forty years ago, on a proposition of Laplace respecting the attraction of spheroids, such a dispute is, we think, without a precedent in the history of mathematics. The puerile contest between the English and the Continental philosophers, in the days of Leibnitz, respecting the force of a moving body, was purely a question of terminology; and not worthy of comparison with the present, except as an example of the mental blindness, in one direction, of the greatest intellects of that age. It is well known that, since the earliest recorded observations, the moon has from century to century been gradually increasing its rate of motion. This acceleration has been traced by Laplace to the secular diminution of the eccentricity of the earth's orbit, from which we conclude that it has been going on for fifteen thousand years past, and will continue for twenty-four thousand years to come. Its amount was calculated by Laplace at about ten

seconds in a century. The calculations of subsequent astronomers gave values varying from 10″ to 12″, the differences proceeding from the different estimates of the fundamental data, such as the distance of the sun, and from the completeness with which the large number of very small terms were included in the final result. These values were found, on the whole, to agree as well as could be expected with certain ancient eclipses, observed at Babylon and elsewhere, of which a few crude observations have been handed down. Thus, up to 1854, the theoretical amount of this acceleration was considered as perfectly determined. But in the Philosophical Transactions for that year Mr. John C. Adams published a paper, contending that the solution of Laplace and his successors was erroneous, and giving a new computation of his own, in which he reduced the amount of acceleration to 6″. It was soon found that M. Delaunay, an eminent French astronomer, who had for many years been engaged on a new theory of the moon, brought out a result exactly agreeing with that of Adams, although his method of investigation was radically different both from that of Mr. Adams and from those of preceding astronomers. This might naturally be expected to settle the question in favor of the new determination. The fact that every step in a course of mathematical reasoning is a vigorous logical deduction, does not preclude all possibility of error, because even the mathematician may, in conducting a very complicated course of analysis, be guilty of some inadvertence involving a serious error, which may remain unnoticed for years. But it does lead us to suppose that, when the error is once pointed out, it will be recognized as an error by every one capable of fully comprehending the reasoning. Several such instances are found in the history of mathematics. In the present case, however, the arguments of Adams and Delaunay, instead of being assented to, were attacked on all sides. De Pontécoulant began by vigorously sustaining the old theory, and in the *Comptes Rendus* of the French Academy and the Monthly Notices of the Royal Astronomical Society poured into the ranks of the innovators a hot fire of mathematical formulæ, and profound distinctions between the different meanings of the term " mean motion."

But a careful recalculation of the acceleration led him to a result which differed from both the old and the new one, being about 8″. This value he vigorously supports. Plana seems to have vacillated considerably, and to have promised an extended memoir on the subject. This paper, so far as we are aware, has not yet appeared. Hansen professes to be entirely satisfied of the accuracy of the old theory, and the consequent inaccuracy of the new one. Le Verrier attacked his associate, M. Delaunay, in person, for his mendacity in bringing out a theoretical result which did not agree with observation. On the other hand, the attacking party has always answered "gun for gun"; and, so far as can be judged from an outside view, has rather the best of the argument. We are not aware that any reply has yet been made to Mr. Adams's paper in the Monthly Notices for May, 1860. So the discussion may, for the present, be regarded as dropped, if not settled.

It seems to be well established that the new theory is inconsistent with the observations of ancient eclipses, and if it should prove to be correct, we may be driven to the conclusion, that a portion of the acceleration proceeds from some other cause than the attraction of gravitation, or that the length of the day is actually increasing to an extent which has become perceptible, from the cause to which we have already referred. If, as centuries roll by, the day should gradually increase, the moon would move a little farther in the course of a day than if no such increase should take place. Since, in our calculations, we suppose the day constant, the apparent acceleration would be greater than the real, — precisely the effect observed. The difference can be entirely accounted for by supposing an increase of something less than one thousandth of a second per century in the length of the day, and a corresponding diminution in the lunar month.

It may be asked, Why have not a sufficient number of mathematicians entered on so singular and interesting a question to decide it in less than seven years? The answer may be found in the general fact that mathematical scholarship is not held in high esteem by mathematicians. The poet, the essayist, and the historian expect their works to be read both by those who are and those who are not themselves poets,

essayists, or historians. The mathematician who writes on such a subject as the theory of the moon cannot reasonably expect his work to be read either by those who are or those who are not mathematicians. The latter class have no taste for it, while the former, if they are able to read the work at all, will prefer making original investigations of their own to poring over those which have been made by others. In no other science is the maxim *Sordet cognita veritas* more fully acted on. A mathematician is estimated, not by what he knows, but by what he has added to the sum total of knowledge. It thus happens that, in a case like the present, although there may be fifty men capable of coming to an independent conclusion, if they would spend time and trouble to read up on the general subject, there are but four or five sufficiently familiar with it to enter into the discussion without much preparation.

We pass now to the consideration of a question which has excited considerable public interest in the course of the past two years. Is there any planet between the sun and Mercury? The history of such a supposed planet is fresh in the recollections of almost every one. Le Verrier announced certain secular changes in the eccentricity of the orbit of Mercury for which he could not account by the attractions of the known planets; he therefore suggested that they might be caused by the attraction of a ring of planets within the orbit of Mercury. He also recommended that observers should systematically note all minute spots on the surface of the sun, with a view of seeing whether they might not be planets in transit. Shortly after the publication of these remarks, in the latter part of 1859, Le Verrier received a letter from Dr. Lescarbault, a physician and amateur astronomer of Orgères (a village about sixty miles from Paris), stating that he had on the 26th of March previous seen an intramercurial planet cross the sun. He gave his observations with considerable particularity, and they were seized upon by Le Verrier as furnishing proof of the existence of the supposed planet. Hypothetical elements were computed from Lescarbault's observations, on the supposition that its path was a circle, and even a name was selected for it and proposed to the *Bureau des Longitudes*. Articles were

published in newspapers and magazines, in which the new planet was spoken of as if it had really been discovered, and as if its existence had been actually predicted by Le Verrier. The public were thus led to believe in its actual existence, and in another great triumph of celestial mechanics in the discovery of an unseen body.

How did astronomers view the matter? At first they were doubtless raised by the sudden wave of evidence into the belief that the supposed planet might actually exist. No reason is known why the series of planets should end at Mercury, and the work of raking up old observations similar to that of the village physician was vigorously prosecuted. Many cases were found in which persons in England and elsewhere had seen spots cross the sun's disk; but, unfortunately, in no case were the circumstances recorded with the detail necessary to furnish data for so testing the observation as to inspire confidence in it. It was soon seen, however, that the new planet was by no means large enough to produce any sensible effect on the orbit of Mercury; indeed, Le Verrier, in stating his original hypothesis, had supposed a group of planets, for the reason that, had there been but one, its magnitude and brilliancy would have been such that it could not have escaped discovery. The more numerous the planets, the more frequently they ought to cross the sun's disk, and thus the more singular it seems that no recognized astronomer should, within the last twenty years, have seen a single transit. Indeed, Lescarbault's planet alone must, according to Le Verrier's elements, cross the sun's disk, on an average, about once every eighteen months. Thirteen transits of that one planet, if it exists, must therefore have occurred within twenty years; and does it seem credible that, if this were so, they would all have escaped the eagle eyes of all other astronomers?

The heaviest blow to Lescarbault's observation came from a quarter whence it was least expected. After several months, copies of scientific periodicals containing the accounts of Lescarbault's observation were borne to Brazil, and there fell into the hands of M. Liais. This gentleman had formerly been an astronomer in the observatory of Paris, but was now chief of

the Coast Survey of Brazil. He at once remembered that he had been engaged, in the spring of 1859, in making careful observations of the surface of the sun. On referring to his records, he found that, at the very time when Lescarbault had, according to his account, been viewing the transit of a planet, he had himself been examining the same part of the sun, without seeing anything of the kind. This would seem to render it reasonably certain that Dr. Lescarbault was in some way mistaken; but what the nature of the mistake might be it is of course impossible to determine. It by no means follows that he was guilty of dishonesty. It is proper to add, that his observation was never recognized by the Academy of Sciences of Paris as leading to a discovery.

The doctrine that gravity is the only force which acts on the heavenly bodies has received a rude blow from late investigations respecting the motions of the tails of comets. These motions were as great a source of perplexity to modern astronomers as the apparitions of the comets themselves were to the ancients. They whisked around with such enormous velocity as to give rise to grave doubts whether they were really material. Certainly their movements could be controlled by no force like gravitation. And why did they always persistently point in the direction opposite that of the sun? It was reserved for Bessel to frame an hypothesis which should account for these anomalous phenomena. In his view, the tail is not a single persistent object carried along with the comet, but a stream of matter, poured from the nucleus, and driven off by the repulsive force of the sun. When the comet is far from the sun, its coldness is such that little nebulous or vaporous matter rises from it; consequently there is little appearance of a tail. As it approaches the sun, and is exposed to a more intense heat, the vapor rises with greater and greater rapidity, and a larger and more splendid appendage is thus formed. Like every other theory which is not a *vera causa*, this must be tested by the completeness with which all its consequences agree with observation. Bessel first made its application, mathematically, to the comet of Halley, in 1835, and found that, by assuming a proper intensity for the repulsive force of the sun, the form and motions of the tail could be represented with considerable fidelity.

Upward of twenty years seem to have elapsed without any serious attempt either to prove or disprove the hypothesis of Bessel. In 1858, the splendid comet of Donati, which for more than a month filled the western heavens with its splendor on every clear evening, excited as much interest among astronomers as among the public. The magnitude, brilliancy, and fineness of outline of its tail, as well as the considerable length of time for which it remained visible, peculiarly fitted it for the application of Bessel's theory. The subject was at once taken up by Professor Peirce, who commenced by deducing more rigorous mathematical formulæ for the motion of the tail than those given by Bessel. He soon found that all the observations could not be satisfied without supposing the repulsive force to be variable for different points of the tail. On the front edge it seemed to be half as strong again as the regular attraction of the sun, while on the back edge it seemed to vanish entirely. With this form of the hypothesis the positions of the tail during a period of several weeks were found to be quite well satisfied. It can scarcely be doubted, then, that Bessel's theory leads to a law which gives the form and position of the tail; in other words, that the tails of comets exhibit the same phenomena that they would if Bessel's hypothesis were true. This, however, does not absolutely prove the hypothesis itself, because it sometimes happens that the same phenomena may proceed from several distinct causes. At least two serious, though not fatal, objections present themselves to the hypothesis.

In the first place, the comet must lose at every revolution a small but appreciable portion of its matter. On any probable hypothesis respecting the age of our system, some of the comets which still exhibit tails must have revolved around the sun millions of times. Why was not all the tail-forming matter contained in them dissipated millions of years ago? In every other branch of the system, all the uncompensated changes are, as we have seen, excessively slow, so as to require enormous periods of time to produce any sensible effect; in this hypothesis, comets would seem to fall out of the range of this rule, if they lose a considerable portion of their matter in every revolution. It may be replied, either that in the begin-

ning all comets were of such prodigious magnitude as not yet to have lost all their volatile matter by the heat of the sun; or that, in the celestial spaces, means exist for restoring the lost material. The adoption of either of these alternatives would, however, weaken the hypothesis. It is a general principle in induction, that every theory which is obliged to assume a number of subsidiary facts or conditions not otherwise known to exist, is, *ceteris paribus*, weakened in proportion to their number, even though no positive objection can be raised against any one of them. This is, in fact, but an application of the law of parsimony. Another answer to the objection is, that we see indications of such a process as the theory supposes. Many bright comets have been seen to pour forth streams of matter from the side of the nucleus next the sun, which matter seemed to spread around the nucleus, and to assist in the formation of the tail. Bright hemispherical envelopes also rise from the same side of the nucleus, and seem to assist in the formation of the coma. The fact that all comets which exhibit a large tail are of long period, is also accounted for by supposing that a small portion of matter is lost by the heat of the sun.

Another objection to the theory is, that we have no evidence that the sun exerts a repulsive force on any other celestial bodies. In the motions of all the planets, and their satellites, it is distinctly seen that the attraction of gravitation is the only force exerted by the sun upon them; and the exertion of a repulsive force stronger than that of gravitation is entirely anomalous. The cause assigned for the supposed repulsion is electricity. If this explanation is correct, the sun must be either positively or negatively electrified to a degree sufficient to make its repulsion felt at a distance of hundreds of millions of miles.

We have thus briefly noted the more prominent arguments for and against Bessel's hypothesis, without pretending to determine which side is entitled to the greatest weight. Until more decisive results are obtained, and the changes which go on in the nuclei and the tails of comets are more minutely ascertained and reduced to law, the opinions of astronomers will differ on this point.